plucked a mussel from the rock knows better in this respect than Dr. Brehm; and when the latter tells us, *à propos* of the songs of birds (p. 37), that the "voice is still motion," and we connect the statement with a previous assertion (p. 19), that "worlds roll on through boundless space—and live," we feel certain that we ought to hear the music of the spheres, or some other mystical sweet sounds, if we could only elevate ourselves to his exalted ecstasy.

But we think we need not trespass further on the time of our readers. We will conclude by expressing the hope that when Mr. Jesse and Mr. Labouchere next set about translating a German author they will have better luck in pitching upon a subject—and they will easily find one—for their labours than the rhapsodies of Dr. Alfred Edmund Brehm.

OUR BOOK SHELF

Proceedings of the London Mathematical Society. Vol. iii., Nos. 21—40.

THE papers read before this Society still preserve the high character attributed to them in the notice of vol. ii., which appeared in this journal. That such should be the case is not matter for surprise, when we run our eyes over the list of contributors. The principal authors are Prof. Cayley and Mr. Samuel Roberts. The former furnishes three memoirs on quartic surfaces (pp. 59—69; 198—202; 234—266); sketch of recent researches upon quartic and quintic surfaces; rational transformation between two spaces (pp. 127—180); on Plücker's models of certain quartic surfaces. The latter communicates papers on the order of the discriminants of a ternary form; pedals of conic sections (pp. 88—98); on the ovals of Des Cartes (pp. 106—126); on the order and singularities of the parallel of an algebraical curve (pp. 209—259); on the motion of a plane under certain conditions. Prof. Clerk Maxwell contributes a paper on the mathematical classification of physical quantities. Besides the foregoing communications, the above-named gentlemen have laid other papers before the Society. Memoirs have also been presented by Mr. J. Griffiths, Mr. J. J. Walker, Prof. Clifford, Hon. J. W. Strutt, and other members. Some other highly valuable communications, we learn from the "Proceedings," were made to the Society, but no record has as yet been made of them, their authors not having yet sent their completed papers for publication. The Society, from the number and high character of its memoirs, seems to have met a want, and is, perhaps, the only Society before which many of the communications could have been brought. As generally the papers are worked out in some detail at the meetings, members have an interesting opportunity of seeing how some of our foremost mathematicians employ their divers instruments. The Society has lost by death during the past session, its first president, and one of its earliest warm supporters. A slight sketch of Prof. De Morgan and his works appeared in NATURE close upon his death in March last. The eighth session of the Society's existence has just commenced, and we trust its future work may be as good as that it has already achieved. *Floreat.*

Treatise on Terrestrial Magnetism. (Blackwood and Sons.)

THE first half of this book contains a good deal of information, and some inquiries connected with the question of the secular variations in the magnetic elements. The author, on the supposition that the secular changes in the declination are caused by the action of a single, slowly rotating pole on a needle which at each place is locally influenced in a definite and determinable manner, computes the declination at several places, and shows that it agrees tolerably well with actual observation. The rotating pole he places at a constant distance of 23° 30′ from the pole of the earth's axis, and gives to its rotation a period of 640 years. The latter part of the book, however, is taken up with "an hypothesis." The writer of this book, and many other such writers, would do well to remember the words of Newton, "*Hypotheses non fingo.*" The hypothesis referred to is simply this:—that the sun attracts the electric matter in the earth and carries it round in a sort of tidal wave, this causes an electric current from east to west, which causes the magnet to point to the north, and from which the writer also attempts to deduce some of the other phenomena of magnetism. There seems to us to be some ambiguity in the writer's method of expression, so that we do not clearly gather whether he intends this current to account for the whole magnetic action of the world, or only for the variations of it. A consideration of the character of the variations of the needle is sufficient to overthrow the hypothesis announced by our author. The solar diurnal variation is thus explained by him:—The pole of the ecliptic revolves once a day round the pole of the earth's axis, the needle tends to follow this, and hence the solar diurnal variation. Now, we may point out a circumstance which, apparently, entirely overthrows, not only this hypothesis, but any which attempts to account for that variation by anything of the nature of the movement of a magnetic pole. At Point Barrow the needle points N.E., at Port Kennedy it points S.W., yet at each place the solar diurnal variation follows local time and exhibits precisely the same features. Standing, then, at the centre of the needle, and looking towards its marked end, that end would at both places be observed to be moving towards the left hand of the observer between the hours of 8 A.M. and 1 P.M. But since the needles are pointing in opposite directions, this constitutes a movement of the marked end of the one towards the geographical west, and of the marked end of the other towards the geographical east, and this at times when the needles are under precisely the same circumstances with respect to the sun's influence. Now, no movement of the magnetic pole can account for this, it would necessarily entail a movement of the marked end of both these needles in the same geographical direction. The consideration of this phenomenon shows us that if the solar diurnal variation of the declination is to be attributed to a current, it must be one not round the magnetic pole or the geographical pole, but along the magnetic meridian. But this is not the place for us to discuss this question further at present. It would seem to be, however, rather from the consideration of such phenomena as this in a careful and accurate way, and the attempt therefrom, by induction, to arrive at laws, that we may hope to form a theory of terrestrial magnetism, than from "making an hypothesis," and then attempting to apply it to facts.

J. S.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Mayer and De Saussure

IN Prof. Tyndall's account of the labours of Mayer, a paragraph is devoted to the bearing of his principles upon the phenomena of vegetable life. It suggests two points of difficulty to me:—

1. It is said that "Mayer's utterances are far from being anticipated by vague statements regarding the 'stimulus' of light, or regarding coal as 'bottled sunlight.'" Nevertheless the paragraph reads almost like a paraphrase of the following passage from Sir John Herschel's "Outlines of Astronomy" (1833), p. 211:—

© 1872 Nature Publishing Group

"The sun's rays are the ultimate source of almost every motion which takes place upon the surface of the earth. By its heat are produced all winds and those disturbances in the electric equilibrium of the atmosphere, which give rise to the phenomena of terrestrial magnetism. By their vivifying action vegetables are elaborated from inorganic matter, and become in their turn the support of animals and men, and the sources of those great deposits of dynamical efficiency which are laid up for human use in our coal strata. By them the waters of the sea are made to circulate in vapour through the air and irrigate the land, producing springs and rivers. By them are produced all disturbances of the chemical equilibrium of Nature, which by a series of compositions and decompositions give rise to new products and originate a transfer of materials."

In a note in Mr. Herbert Spencer's "First Principles" (2nd Ed., p. 496), which first led me to look at this passage, it is remarked that Herschel "expressly includes all geologic, meteorologic, and vital actions, as also those which we produce by the combustion of coal," in the effects of the solar rays. When, therefore, Prof. Tyndall states that Mayer *revealed* the source of the energies of the vegetable world, it appears to me that Herschel anticipated the revelation twelve years previously. Of course, I apprehend that Mayer's merit consisted in seizing at once a physical principle of immense generality, and in applying it to very different phenomena. Herschel began at the other end; but appears equally to have seen the solar energy underlying these phenomena, though in a general way, and without demonstrating numerical relations.

2. De Saussure is credited unreservedly with the observation of the reducing power of the solar rays in the vegetable economy. But he seems to me, as, indeed, he seemed to himself, to have only crowned a theory which other workers had elaborated. Priestley began by ascertaining that air depurated by animals was purified by plants. Ingenhousz showed, what Priestley candidly confesses he missed, that this effect is due "chiefly, if not only, to the light" of the sun. Senebier found that "fixed air" was the ingredient which plants removed from a vitiated atmosphere, and that this underwent elaboration in the tissues, oxygen being set free as the result.[*] Lavoisier having previously shown that fixed air was a compound of carbon and oxygen, Senebier's results implied the fixation of carbon by plants. This fixation De Saussure actually demonstrated by feeding a plant with carbon dioxide and water alone, and showing that the carbon in the tissues increased. He further found the unexpected fact (and this is what he added to the matter) that the oxygen evolved by plants does not correspond to that contained in the carbon dioxide absorbed, but that it is smaller in quantity.

De Saussure's researches are a beautiful example of quantitative work, but they would have, I imagine, merit of a different order if Priestley, Ingenhousz, Senebier, and Lavoisier had not broken ground before them.

W. T. THISELTON DYER

Phenomena of Contact

IN NATURE of August 24 I objected to, as misleading, the statement by Mr. Newcomb that "we find ligaments, black drops, and distortions sometimes seen in interior contacts of the limbs of Mercury or Venus with that of the sun, described as if they were regular phenomena of a transit, without any mention of the facts and experiments which indicated that these phenomena are simple products of insufficient optical power and bad definition which disappear in a fair atmosphere with a good telescope well adjusted to focus." I asked for references to the facts and experiments by which the statements are justified.

In NATURE of September 28 I find Mr. Newcomb's reply, but without the references which I desired. Mr. Newcomb considers that I controvert the two following propositions:—

1. That the irregular phenomena of internal contact of a planet with the sun, variously described as distortions, black drops, ligaments, &c., are not always present, but are only seen sometimes.

2. That when seen they are due to insufficient optical power or bad definition.

If the word "irregular" is cut out, and the word "seen" substituted for "present" in proposition (1) there can be no doubt about its truth. It will be found that all the arguments adduced by Mr. Newcomb to prove this proposition have no bearing either upon the word "irregular" or "present" in contradistinction to "seen."

It appears to me, therefore, quite unnecessary to allude further to this proposition.

With reference to proposition (2), I believe it to be utterly erroneous. I believe that the phenomena of the fine connecting ligament can only be seen in a fair atmosphere, with a good telescope well adjusted to focus, and with considerable magnifying power. When it is remembered that the fine connecting ligament is confined to within about a second of arc of the sun's limb, I think my statement will at least commend itself to practical observers. Mr. Newcomb appears to regard it as a great difficulty in my view of these phenomena, that some of the observers should see the ligament and some not. I am rather surprised at the persistence with which this point is again and again brought forward in his letter. I thought that it had been answered by anticipation in my letter which appeared in your number of August 24. In all my writings upon the subject I have maintained that the phenomena could only be seen under favourable circumstances and with sufficient power; and in my letter of August 24 will be found this statement, which appears to have been entirely overlooked, at least unanswered, by Mr. Newcomb:—"The optical enlargement by irradiation is a function of the brightness, and can be made insensible by sufficiently diminishing that brightness. Unfortunately, however, when this diminution of brightness is carried to a very great extent, errors in an exactly opposite direction to those of irradiation will come into play, similar, in fact, to the results of Wolf's experiments. The observations of Mercury on the sun's disc in 1868 were made with very different optical means, and some very different methods were adopted for diminishing the sun's glare." In my view those observers who did not see the connecting ligaments failed to see it, either from want of attention to the point as not a contact such as they expected to see, or from the observations having been made under such circumstances that some of the necessary conditions which I have indicated were not satisfied.

The fine connecting ligament is only seen by contrast against the illumination of the sun's disc near the point of contact, and it may well be that some of the observers have pushed the diminution of brightness of the sun's image to such an extent that the contrast was too feeble to attract attention before the apparent contact. To me, and I think to others who will give the matter some consideration, it is clear enough "that an observer with the naked eye, a telescope of low power, would not, in the case of a transit of Venus, see the connecting ligament at all." It is as clear "that without seeing any ligament, the planet, at egress, would appear to touch the limb without distortion, necessarily, earlier than the contact would appear to be established to observers who were watching the transit with good telescopes and with high powers." It appears to me equally clear that, if the brightness of the sun's image be reduced to excess, then the ever-diminishing small portion of the illuminated disc between the sun's edge and the advancing planet, at egress, may be made to disappear, from sheer inability to appreciate so faint a light, before the contact would appear to be established to observers who had not so reduced the brightness of the image. Disturbing causes such as these do exist, and their effects must be recognised. I must apologise for bringing forward such arguments; but, since one observer has published his opinion that the "ligaments, &c.," do not exist in contacts, because he looked at the transit of Mercury with an opera glass and saw nothing of the kind, it would appear necessary to recall attention to a common-sense view of the points at issue.

But whatever may be the opinion of Mr. Newcomb respecting the explanation which I have given of the probable reason why some of the observers have not seen the connecting ligament, he must feel that it will at least be difficult for him to explain away the positive evidence of the numerous observers who profess to have seen the ligament with first-rate telescopes. Some of them, at least, were gentlemen not likely to have forgotten to adjust their eye-pieces to focus, even if such a neglect would have produced the phenomena observed. In cases where the ligament has been seen, it will be found that the earlier lines of contact at egress have been given by observers with the best telescopes and high powers. This is strikingly shown by the Greenwich observations of the transit of Mercury, 1868. It is a result in perfect accord with my views. Very large and systematic discordances will be found to exist between the times of internal contact at the transit of Mercury, 1868, in cases where no connecting ligament was seen at all. This has been passed over in

---

[*] Recherches sur l'influence de la lumière solaire pour metamorphoser l'air fixe en air pur par la végétation, 1783.

© 1872 Nature Publishing Group